

The first of these stories is related in very circumstantial terms, but without any authority being given for it. It is said that a Fellow of the Geological Society offered a certain paper, which the authorities of the Society refused; and it is asserted that the reason of their refusal was that the paper "was not orthodox," and "they probably smelt heresy." Now the Duke of Argyll is well aware that every Fellow of the Geological Society has the right to present papers for reading, and that the responsibility for accepting or refusing papers rests in the first instance with the President; but he, in the case of exercising his veto, is bound to report the fact, and the reasons for his action, at the next meeting of the Council. The records of the Society show that no such paper was ever offered to it; that the President never exercised his right of veto; and that the Council never discussed the grounds of the supposed refusal. The Duke of Argyll has been informed of these facts, but he has not yet retracted the very serious charge which he has made affecting the honour and good faith of the President and the other twenty-two members of the Council of the Geological Society.

In the case of the second story circulated by the Duke of Argyll, the authority is given. The complaint is made that since 1862 "advanced geologists" have "ignored" views which "tend to dethrone" their own "pet theories." Anyone who chooses to refer to the *Philosophical Magazine* for 1862 will see that the "pet theories" in question are those relating to the antiquity of man; that the "advanced geologists" implicated in the charge must have been the late Sir Charles Lyell, Prof. Prestwich, and those who have followed up their researches and arguments; and that the "views" which they "ignored" were the suggestions which I described in my last letter!

JOHN W. JUDD.

The Total Eclipse of the Moon of 1888 January 28, as observed at Birr Castle Observatory, Parsonstown.

THE total eclipse of the moon on Saturday last was, like its predecessor in 1884 (see *NATURE*, vol. xxx. p. 589, and *Trans. Royal Dublin Society* for October 1885), favoured by a very clear sky during the whole time of its progress, so that very extensive observations of the changes of the moon's heat in consequence of the passing over of the earth shadow could be made. The apparatus used was essentially the same as that used before; yet the two old thermopiles had been replaced by two new ones especially made for this occasion by the Earl of Rosse.

The observations began at 7h. 19m. M.T. Greenwich, and were, as much as possible, uninterruptedly continued till 15h. 45m.

During this time 638 distinct readings of the galvanometer were obtained, which, when fully reduced, will enable a very satisfactory heat-curve to be drawn. A few preliminary results, reduced to zenith, I communicate at once.

Galvanometer.

739.4	...	1h. 10m. before first contact with penumbra.	
663.4	...	24m. "	"
624.1	...	First contact with penumbra.	"
252.1	...	" shadow.	"
34.9	...	22m. before beginning of total phase.	"
30.2	...	22m. after "	"
231.9	...	Last contact with shadow.	"
545.6	...	" penumbra.	"
540.8	...	1h. 34m. after last contact with penumbra.	"

From these figures it will be seen—

(1) That the heat radiated by the moon begins to decrease a considerable time before the first contact with the penumbra.

(2) That 22m. before the beginning of totality the heat is only 4.7 per cent. of the value obtained 1h. 10m. before the first contact with the penumbra. Unfortunately an unforeseen stoppage of the driving-clock prevented the observations from being carried on closer up to and during the total phase.

(3) That in spite of the rapid fall on approach to totality, the heat, after the last contact with the penumbra, does not at once increase to anything like the value observed at corresponding times before the first contact.

It is worth remarking that points 2 and 3 are confirmatory of the results arrived at in 1884.

OTTO BOEDICKER.

Birr Castle Observatory, Parsonstown, January 30.

"Elementary Chemistry," and "Practical Chemistry."

I CRAVE leave from the Editor for space in which to reply, on my own behalf and on that of my fellow-authors Messrs. Slater and Carnegie, to the charges brought by "H. E. A." in *NATURE* of January 19 (p. 265) against our method of teaching chemistry. At the outset I thank "H. E. A." for the patience which, as he publicly announces, he has shown in waiting for the publication of these books, and I condole with him in his disappointment. Like him, I too am waiting patiently; I trust my disappointment will be less bitter.

One of the important points in our plan of chemical teaching is the connection of the work in the laboratory with the student's reading and lecture-work. To emphasize this connection, and to make our course run fairly smoothly, we have published two books, one to be used in the laboratory, the other to be used in the lecture-room and in reading in connection with the whole work of the student. "H. E. A." acknowledges the advantages of this division, but throughout his review he ignores the statement distinctly made by us, that one book is complementary to the other and that both must be used together. He confines his remarks almost wholly to one of our books, viz. the "Practical Chemistry"; and yet he condemns our system of teaching. On this ground alone I claim that his review is misleading and unfair. I go further, and assert that "H. E. A." has condemned our system without acquainting himself with its essential features. He says that "in the earlier part of the 'Practical Chemistry' Messrs. Muir and Carnegie do not sufficiently bear in mind their own intention, and that much of the matter would find a more fitting place in the companion volume." No one reading this would suppose that almost every experiment used in Chaps. I. to VIII. of the "Practical Chemistry" is also used in Chaps. I. to IX. of the "Elementary Chemistry." Yet this is the case. In one book the experiments are described, along with others, in such terms as allow attention to be concentrated on their results and on the reasoning on these results; in the other book the experiments are described in detail in order that the student may repeat them in the laboratory. In another part of his review "H. E. A." says that most of the subjects dealt with in the third part of the "Elementary Chemistry" "ought never to have been introduced into an 'Elementary Chemistry.'" He has here made a slip: it is the third part of the "Practical Chemistry" which includes subjects not touched on in the other book. This correction involves a point of some importance. Although the preface to our "Practical Chemistry" states that the book forms part of a course of elementary chemistry, yet the student who uses both books will see that the course of work laid down in the practical book carries him much beyond the limits of treatment adopted in the other volume. There are numerous direct and indirect indications of this in the book itself, which those for whom the work is intended will not fail to notice. One cannot put the whole of one's book into the preface. I admit that it would have been better had we indicated in the preface to the "Practical Chemistry" that many experiments in Parts II. and III. are difficult to perform, and require skill and training; but I assert that the nature of the experiments themselves, the references to the original papers to be read before conducting these experiments, and the suggestions as to other work to be done preparatory to Parts II. and III. respectively, suffice to indicate to the student, although not necessarily to the reviewer, the character of the work described in the later chapters of the "Practical Chemistry."

Chapter I. of Part III. of the "Practical Chemistry" involves a repetition of some of Stas's determinations of the atomic weight of silver. "H. E. A." says that this chapter should have been included in Part I., and he adds, "the remaining chapters ought never to have been introduced into an 'Elementary Chemistry,'" kindly informing his readers that these chapters are included "because of the senior author's well-known tendency to worship physical constants." I venture to remind "H. E. A." that no election has taken place to the office of supreme pontiff of chemistry. Were that official in existence, I feel inclined to think he would admit that accurate determinations of atomic weights—and "H. E. A." allows these in the most elementary part of the course—are determinations of constants which have physical as well as chemical meanings.

"H. E. A." says that in the "Practical Chemistry" there is an "entire absence of anything approaching to a systematic arrangement." The boldness and baldness of the assertions made by the reviewer encourage me to meet this statement with

a direct denial. There is a systematic arrangement in the whole book, or rather in the whole scheme embodied in both books. Because "H. E. A." fails to discover that plan which finds favour with him, it does not follow that systematic arrangement is absent. To say that the arrangement is not that which one would like to see adopted is fair criticism; but to imply that there is no alternative between one's own system and chaos is to expose one's own ignorance. And what is the feature of that system of practical chemistry in which alone the reviewer thinks the student can find salvation? He says, "in a properly chosen series of experiments everything should be proved; no assumption should be necessary." In another part of the review he tells us that "air and the phenomena of combustion should be first studied: the composition of air should be determined, and oxygen should be discovered by the student . . . The composition of water should next be qualitatively ascertained." I should be deeply indebted to "H. E. A." if he would kindly describe experiments on these subjects, suited to beginners in chemistry, in which no assumptions are made, and which convey sound teaching. He must not get over the difficulty by cleverly hiding the assumptions made, and so appearing to make none; everything must be proved, and proved by experiments which the beginner can satisfactorily conduct. I hold, and I am convinced that the history of science bears me out, that all scientific reasoning starts with certain assumptions, and that in every particular train of reasoning assumptions are made. If the beginner can be taught to recognize the assumptions which are involved in his reasoning on experimental data, he will do well. In the "Practical Chemistry" we have tried to emphasize the assumptions which the beginner must make. In our opinion the fatal thing is to cover over and hide away the assumptions; by doing this, the student acquires a habit of confounding hypotheses with facts, and so unconsciously he slides into loose methods of reasoning. I fancy I can detect the effects of such a method in the whole review: has not "H. E. A." tacitly, probably unconsciously, assumed that chemical truth abides with him and with him only?

We thank "H. E. A." for indicating some points in the descriptions of certain experiments which might be improved, and also for reminding us that the drawings of apparatus are not as good as they might be. These things can and will be improved. The mistake in the description of the diffusion-experiment, on p. 30 of the "Elementary Chemistry," to which "H. E. A." alludes, has been already pointed out to us, and a slip has been inserted in all copies except the first few hundred correcting this mistake. We cannot congratulate the reviewer, nor do we think he will be inclined on second thoughts to congratulate himself, on the trifling quibbles in which he has indulged regarding one of our experiments on the electrolysis of water.

Cambridge, January 23. M. M. PATTISON MUIR.

"Physical Science and the Woolwich Examinations."

I AM afraid that the moderation of your article on the regulations for admission to the military colleges may give some readers the impression that science is merely being discouraged more or less seriously in their examinations. The fact is, however, that it is being ousted with absolute certainty, for hardly anyone can afford to take up an optional subject which is at a disadvantage of 1000 marks. Severity of competition has within the last few years quite doubled the number of marks qualifying for admission to Sandhurst, and it will soon be impossible, even if it is not so at present, for a candidate to gain a place if he takes up any subject other than Latin, French, German, or mathematics.

This making all the men fit square holes whether they are round or not can hardly be for the advantage of the service, and one's curiosity is aroused as to the reason for such retrogressive changes—whether it is due, as has been asserted, to the action of head masters who do not desire to accumulate or encourage new-fashioned lore; or whether the military authorities really opine that to an officer who may have to deal with telegraphy, to choose a ramping-ground, or perhaps direct a search for water, Latin is half as important again as electricity or physical geology.

Is it really too much to expect that they might insist first on a thorough knowledge of those parts of an ordinary education which are specially necessary or helpful to an officer, and then treat the unessential subjects on an equality as far as possible, and let a boy do in his preparation as he will when a man—

adequately fulfil the duties of his position, and then follow his own bent?

W. A.

January 30.

"The Art of Computation for the Purposes of Science."

HAVING read with much interest Mr. Sydney Lupton's second article on this subject, I think it right to draw his attention, and that of your readers, to Table III. of my book of five-figure and other logarithms published by Messrs. C. and E. Layton in 1870.

This table was framed by me for the purpose of enabling computers who occasionally require to use logarithms to ten places to get same with as little trouble as possible, and without shifting to any other book. In fact, I believe results can be got from my table almost as quickly as from the voluminous and beautiful volume of George Vega.

For instance, referring to Mr. Lupton's example, I find from my table and the instructions that $\log 1.0542482375 = \log 1.05 + \log 1.0040459405$ —this by simple division; then—

$$\text{By part A } \log 1.05 = 0.0211892991$$

$$\text{By part B } \log 1.0040459405 = 0.0017535845$$

$$\log 1.0542482375 = 0.0229428836$$

correct by Mr. Lupton's solution from Vega.

My whole table is contained in eight octavo pages, and I believe is in as narrow a compass as is consistent with utility.

I may add that in the preliminary part of my book will be found a method of finding the logarithms of all numbers by nothing more than simple multiplication.

The late Prof. Augustus De Morgan, when I showed him this Table No. III., I well remember, replied: "It is very good indeed, but you will get no one to look at it," showing how rarely logarithms are ever required for any practical use beyond five, or at the most seven, figures.

E. ERSKINE SCOTT.

6 Bond Court, Walbrook, London, E.C.,

January 18.

THE articles of Mr. Sydney Lupton on the above subject, which have appeared in recent numbers of your paper, do not profess to be complete; still, as their declared object is to assist those who are not mathematicians to work sums by the aid of tables, it seems to me that the best methods should not be passed over in silence, while others that are practically obsolete are discussed at length.

I beg of you therefore to allow me to call attention to the labours of the late Peter Gray, F.R.A.S., in the direction of supplying facilities for computing logarithms and antilogarithms. He contributed papers on the subject to various magazines; notably a series (with a table for formation of logarithms and antilogarithms to twelve places) to the Journal of the Institute of Actuaries in 1865. His most important work on this subject was, however, published as an independent volume in 1876. It is entitled "Tables for the Formation of Logarithms and Antilogarithms to Twenty-four or any less number of places"; and it contains, besides the tables, an explanatory introduction and an exhaustive historical preface. The published price is only 7s. 6d., and it is therefore not beyond the reach of those who require such tools.

Weddle's method, the last mentioned by Mr. Lupton, consists in multiplying the given number down to unity, by means of a series of factors of the form $1 - (1)^n \times r$, where r may take any integral value from 1 to 9. The logarithms of the factors are then obtained from a previously prepared table, and the complement of the sum of these logarithms is the logarithm of the given number. Weddle also used his method conversely, to calculate antilogarithms.

Hearn, of the Royal Military College, Sandhurst, improved upon Weddle's method, by substituting factors of the form $1 + (1)^n \times r$ for the computation of antilogarithms, r , as before, ranging in value from 1 to 9; but he retained the factors $1 - (1)^n \times r$ for computing logarithms.

Gray's improvements on Hearn were twofold. In the first place, he gave r the range from 1 to 999, taking for factors $1 + (001)^n \times r$, and he thereby brought within narrow compass the arithmetical work involved. In the second place, by a simple arrangement of the calculations, he showed how to use factors of the form $1 + (001)^n \times r$, instead of $1 - (001)^n \times r$,